This is an engaging and closely reasoned essay on the role of intuition in analytic philosophy. Although Deutsch’s primary target is experimental philosophy (xphi), he argues that the myth of the intuitive has been around longer than xphi and that some of the myth’s most influential supporters (e.g. George Bealer, Laurence Bonjour, Alvin Goldman, Frank Jackson, and Joel Pust) are not experimental philosophers. One of the virtues of this book is Deutsch’s effort to treat xphi fairly by
distinguishing different uses of experimental methods and giving credit where he thinks credit is due. His criticism of xphi focuses on what he calls “negative xphi.” According to Deutsch, negative xphi collects data on people’s intuitions and how they vary from group to group in order to: “call certain traditional philosophical arguments into question and, most importantly, to raise a worry about the epistemic value of the philosophical intuitions negative xphiles take to be involved in those arguments.” (pp. 1-2) He maintains that the flaw in this project is its false assumption that traditional philosophical arguments depend on intuitions as evidence. Although I have deep reservations about some of Deutsch’s conclusions, I believe his argument helps to illuminate the methodological clash between partisans of the armchair and practitioners of xphi.

Early in the book Deutsch draws a distinction between two ways of understanding the claim that intuitions count as evidence for philosophical arguments:

(EC1) Many philosophical arguments treat the fact that certain contents are intuitive as evidence for those very contents.

(EC2) Many philosophical arguments treat the contents of certain intuitions as evidence for or against other contents (e.g. the contents of more general principles.) (p. 36)

Deutsch believes that EC2 is true and EC1 is false. He asserts that “we see confirmation of EC2 in nearly every case in which a philosopher constructs a counterexample to a generalization or theory” (p. 37), but, outside of metaphilosophy, rarely see philosophers appealing to the fact that an intuition that p is an intuition as evidence that p is true. He contends that in first-order philosophy (as opposed
to metaphilosophy) philosophers appeal to the justifiability of some of the propositions we intuit rather than to the fact that they are intuitions. In other words, it is the content of an intuition that matters, not its state as intuition.

As Deutsch correctly observes, analytic philosophers do not agree on how the term ‘intuition’ should be used. Some insist on distinguishing characteristics like being spontaneous, non-perceptual, or non-inferential. Bealer, for example, defines intuitions as intellectual seemings and, in Deutsch’s words, “thinks that philosophical intuitions present their contents as necessarily true.” (27). Others just equate intuitions with judgments, beliefs, or dispositions to believe. Deutsch embraces a “no-theory theory of intuitions.” Rather than characterizing intuitions or intuitiveness in advance, he takes instances of judgment that philosophers have called or treated as intuitions and tries to assess their evidential import for philosophical theorizing. While this seems to me a reasonable strategy, I suspect that his reliance on the term ‘intuitiveness’ compromises his consistency in sticking to it. ‘Intuitiveness’ can be used to mark the likelihood of a particular belief or judgment (e.g. “it isn’t knowledge”) arising when people consider a given case (e.g. a Gettier case), but it can also be used to refer to an epistemic property of a belief or judgment. I think he lets the latter bleed into the former. I try to be more consistent in this review by avoiding ‘intuitiveness’ except to quote Deutsch and by using ‘intuition’ interchangeably with ‘judgment’—leaving open the question of whether a given intuition/judgment is spontaneous or non-spontaneous, inferential or non-inferential, reliable or non-reliable, etc.

Deutsch builds his case against negative xphi and the myth of the intuitive by a close examination of some well-known thought experiments that philosophers have proposed to
challenge various definitions, theories and principles. He discusses at length Gettier’s counterexamples to the definition of knowledge as justified true belief and Kripke’s “Kurt Gödel” counterexample to descriptivist theories of reference for proper names. He examines more briefly Frankfurt’s counterexample to the principle of alternative possibilities (PAP) and several other counterexamples as well. He considers both the explicit wording of these thought experiments and, what he takes to be, their implicit methodology. For the sake of economy, I concentrate below view on Gettier, Kripke, and Frankfurt.

A well-drawn thought experiment is effective when it elicit the intuition it was designed to elicit, but much the same may be said of well-drawn optical illusions Why trust that the elicited intuition is true? Deutsch’s thesis is that the evidence for its truth comes from reasons and arguments suggested by their proposers and defenders. He contrasts his thesis with Timothy Williamson’s view that no evidence is required beyond the believability of the contents of the intuition. He also contrasts his thesis with, what he takes to be, the commitment of negative xphi to count as evidence how many or what groups of people have the intuition.

In support of his thesis he points out that Gettier, Kripke, and Frankfurt all give arguments for accepting the truth of the intuitions their thought experiments are designed to elicit and that subsequent defenders add arguments of their own. Gettier’s arguments are very brief, but, then, so is the paper in which they appear. Both of his thought experiments are designed to elicit the intuition that a particular instance of belief, though justified and true, is not knowledge. In the Ten Coins Case, Gettier gives the following argument for concluding that proposition (e)
“Smith knows that the man who will get the job has ten coins in his pocket” is false.

But it is equally clear that Smith does not know that (e) is true; for (e) is true in virtue of the number of coins in Smith's pocket, while Smith does not know how many coins are in Smith's pocket, and bases his belief in (e) on a count of the coins in Jones's pocket, whom he falsely believes to be the man who will get the job. (p. 122)

In the Brown in Barcelona Case, Gettier gives the following argument for concluding that proposition (h) “Smith knows Jones owns a Ford or Brown in Barcelona” is false.

But imagine now that two further conditions. First Jones does not own a Ford, but is at present driving a rented car. And secondly, by the sheerest coincidence, and entirely unknown to Smith, the place mentioned in proposition (h) happens really to be the place where Brown is. If these two conditions hold, then Smith does not know that (h) is true. . . (p. 123)

Deutsch then goes on to say:

The presence of these arguments shows that the idea that Gettier relies only on intuition in support of his judgments about his cases is a myth, a piece of philosophical folklore. . . But an examination of the paper reveals that Gettier does not anywhere appeal to intuitions or the intuitiveness of the judg-
ments he makes about his cases. He relies instead on arguments. (p. 83)

Although Deutsch is correct in saying that Gettier does not mention intuitions or intuitiveness, he cannot say the same about Kripke. Early in _Naming and Necessity_ (1980) Kripke declares:

> Of course, some philosophers think that something’s having intuitive content is very inconclusive evidence in favor it. I think it’s very heavy evidence in favor of anything myself. I really don’t know, in a way, what more conclusive evidence one can have about anything, ultimately speaking. (p. 42)

Deutsch claims that it is a mistake to read this passage as an endorsement of intuitions _per se_ as evidence. He argues that the subject here is the distinction between essential and accidental properties and that “all that Kripke means by the claim that the distinction ‘has intuitive content’ ” is that ordinary people draw the distinction. (p. 105) Although I find this reading implausibly narrow, I concede there is room to debate how Kripke understood his remark about the evidential primacy of intuitions to apply to the Gödel Case. So let’s put that remark aside and examine his presentation of the Gödel Case. This case is intended to counter semantic theories that claim that the person to whom a personal name refers is determined by descriptions associated with that name. This case is designed to elicit the intuition that even under the bizarre conditions specified, the name ‘Gödel’ still refers to Gödel, who does not fit the operative description, rather than to Schmidt, who does fit the operative description. Here is how it goes.
Kripke asks us to imagine that the mathematician Kurt Gödel did not in fact discover the incompleteness of arithmetic. He asks us to imagine: “A man named ‘Schmidt’ whose body was found in Vienna under mysterious circumstances many years ago, actually did the work in question. His friend Gödel somehow got hold of the manuscript and it was thereafter attributed to Gödel.” (p. 84) The example is clever because most people, if they have heard of Gödel at all, have only heard about him as the man who discovered the incompleteness of arithmetic. So a descriptivist account of what ‘Gödel’ refers to might plausibly be reduced to: ‘the man who discovered the incompleteness of arithmetic.’ (This is what I have called the operative description.) Kripke then says the following:

On the [descriptivist] view in question, then, when our ordinary man uses the name ‘Gödel,’ he really means to refer to Schmidt, because Schmidt is the unique person satisfying the description ‘the man who discovered the incompleteness of arithmetic.’ . . . So, since the man who discovered the incompleteness of arithmetic is in fact Schmidt, we, when we talk about ‘Gödel,’ are in fact always referring to Schmidt. But it seems to me that we are not. We simply are not. (p. 84)

Although the last sentence of this quote suggests that Kripke is appealing to brute intuition, he also offers an argument in a footnote. After sketching the fictional Gödel example, he gives some real-life examples of people using a name “on the basis of considerable misinformation.” (p. 84) Some people believe that Peano discovered the so-called Peano axioms, though in fact it was Dedekind. Some people believe that Einstein invented the atomic bomb, and
some believe that Columbus was the first man to realize that the Earth is round. A descriptivist theory implies that when people who are misinformed in these ways use the names ‘Peano,’ ‘Einstein,’ or ‘Columbus,’ they are referring to the individuals who actually accomplished these things. “But they don’t,” insists Kripke. He adds in a footnote that if they were to say: “Peano discovered Peano’s Axioms” or “Einstein invented the atomic bomb” or “Columbus was the first man to realize the Earth is round,” their statements would be false, not trivially true. (p. 85n) This is where Deutsch locates the evidence for the evidence and anchors his brief against x-phi challenges to Kripke’s interpretation of the Gödel case.

In 2004, 24 years after the publication of Naming and Necessity, Edouard Machery, Ron Mallon, Shaun Nichols, and Stephen Stich published the results of their experimental research on the Gödel case under the title: “Semantics, Cross-Cultural Style.” The subjects of the experiment were 82 undergraduate, 40 culturally Western students at Rutgers University and 42 culturally Chinese students at the University of Hong Kong. These students were asked to respond to four (verbal) probes modeled on examples from Naming and Necessity. Two of the probes were modeled on the Gödel/Schmidt thought experiment. One uses some of Kripke’s own wording of the Gödel/Schmidt story, and the other uses a parallel story about the Chinese astronomer Tsu Ch’ung Chih who is famous in China for having discovering the precise times of the summer and winter solstices. The former probe describes a speaker named John who learned in college that Gödel was the man who proved the incompleteness arithmetic and has never heard anything else about Gödel. It also asserts that, unbeknownst to John, Schmidt really discovered the incompleteness of arithmetic, though Gödel got hold of the manuscript and claimed credit for the work.
Finally, it asks the participant to answer the following question: “When John uses the name ‘Gödel’ he is talking about: (A) The person who really discovered the incompleteness of arithmetic? (B) The person who got hold of the manuscript and claimed credit for the work?” The latter probe asks a parallel question about Tsu Ch’ung Chih, substituting the discovery of the solstices for the discovery of arithmetic’s incompleteness.

The Western students picked B answers, the causal-historical answers, nearly twice as often as the Chinese students:1.13/.63. Machery et. al. interpret this result as bearing out their prediction that, in light of earlier studies on East-West cultural differences, Westerners would be more likely than Chinese to have intuitions that fall in line with non-descriptivist (i.e. causal-historical) theories of reference. “Thus,” they say, “the evidence suggests that it is wrong for philosophers to assume a priori the universality of their own semantic intuitions.” (p. 53) They conclude the article with a sweeping admonition:

Since the intuitions philosophers pronounce from their armchairs are likely to be a product of their own culture and their academic training, in order to determine the implicit theories that underlie the use of names across cultures, philosophers need to get out of their armchairs. (p. 54)

Subsequent experimental studies of Gödel-type cases have yielded somewhat different results, but the methodological issues remain the same. Deutsch vents his frustration with those who think that essential evidence for a theory of references resides in how many or which sorts of people have intuitions consonant with that theory. He says:
The view that it is simply an intuition about the Gödel Case that is the engine driving Kripke’s view of the case is a bad mistake. A worse mistake is thinking that the cross-cultural variability about the case, or ones similar to it, presents a significant challenge either to Kripke’s judgment about the case or to the methods he uses in arriving at it. Instead, the judgment stands or falls with the quality of the arguments marshaled in its defense. The worst mistake is to think, as Machery, Mallon, Nichols, and Stich put it in their most recent paper [2012], that Kripke and other philosophers of language think that “the correct method for determining the right theory of reference” is “by appeal to the intuitions of competent speakers about the reference of proper names (or other kinds of words) in actual and possible cases.” . . . No. This is just plain wrong. (p. 111)

Deutsch gives less attention to Frankfurt’s equally famous thought experiment concerning moral responsibility, but he draws the same methodological conclusions. In “Alternate Possibilities and Moral Responsibility” (1969) Frankfurt challenges the principle that a person is morally responsible for an action only if that person could have done otherwise (PAP) by presenting variations on a thought experiment designed to elicit the intuition that: “since Jones is acting solely from reasons of his own, the fact that Black ensures he cannot act otherwise does not excuse him from moral responsibility.” Yet Deutsch denies that Frankfurt is thinking in terms of intuitions or intuitiveness at all. He states:
You were asked earlier to accept that the answer is: the intuitiveness of the claim. But this not the answer that Frankfurt actually gives, and there is no reason to think that it is implicit in Frankfurt’s discussion. (He does not use the term “intuition” or its cognates, for example.) (p. xv)

In fact this denial conflicts with Frankfurt’s own words. Near the beginning of the essay Frankfurt says, “I propose to develop some examples . . . and to suggest that our moral intuitions concerning these examples tend to disconfirm the principle of alternate possibilities.” (p. 830)

Although Deutsch is mistaken about Frankfurt’s use of the term ‘intuition,’ Frankfurt’s does indeed give an argument for the intuition his case is designed to elicit. The common denominator of Frankfurt’s variations is a counterfactual intervener (Black) who guarantees that a person (Jones) could not do otherwise but in fact plays no role in bringing about that person’s action, since the person (Jones), acts solely for reasons of his own. His argument is:

It would be quite unreasonable to excuse Jones for his action or to withhold the praise to which it would normally entitle him, on the basis of the fact that he could not do otherwise. This fact played no role at all in leading him to act as he did. He would have acted the same even if it had not been a fact. Indeed, everything happened just as it would have happened without Black’s presence in the situation and without his readiness to intrude into it. (p. 836)
He adds in a footnote that the role played by Black could be played by “natural forces involving no will or design at all.” (p. 836n) Again Deutsch stresses that what is going on here is not merely the eliciting of the intuition “since Jones is acting solely from reasons of his own, the fact that Black ensures he cannot act otherwise does not excuse him from moral responsibility.” Frankfurt, he claims, gives evidence for the truth of that intuition (“evidence-for-the-evidence”) by pointing out that Jones’s inability to do otherwise “played no role at all in leading him to act as he did” and then “evidence-for-the-evidence-for-the-evidence” by pointing out “everything happened just as it would have happened without Black’s presence in the situation and without his readiness to intrude into it.”

Toward the end of The Myth of the Intuitive Deutsch consider several replies to x-phi that differ from his primacy of arguments thesis. Two are especially interesting. The “expertise reply” takes x-phi to task for gathering data on folk intuitions (the intuitions of ordinary people) rather on what really counts: the intuitions of philosophers. Deutsch agrees that the judgments of philosophers are likely to be better than the judgments of non-philosophers, but not because philosophers are better intuiters. “They are better,” he says, “because philosophers are better than nonphilosophers at arguing for, and defending, philosophical judgments.” (p. 141) The “multiple concepts reply” suggests that diversity among intuitions in response to thought experiments may be the result of different people drawing on different concepts—say, of knowledge—and, therefore, disagreements between them of whether x is an instance of knowledge are apparent rather than real. Here Deutsch sides with x-phi: the burden of proof is on the proponents of multiple concepts.
Deutsch’s main thesis is refreshingly direct, but is it sound? “Philosophy,” he says, “simply does not rely evidentially on intuitions or what is intuitive. Instead philosophers argue for their judgments about cases, and the cogency of these arguments is independent of who intuits them.” (p. 155) My principal reservations about Deutsch’s thesis and the arguments he musters in its favor are given below.

I doubt that the arguments he attributes to Gettier, Kripke, and Frankfurt for the truth of the intuitions elicited by their cases are clearly distinguishable from the cases themselves and therefore constitutes “evidence for the evidence.” When, Gettier argues for the conclusion that Smith doesn’t know (e) (“The man who will get the job has ten coins his pocket”), he restates what he has already stipulated to be true in this case: 1) “(e) is true in virtue of the number of coins in Smith's pocket”; 2) “Smith does not know how many coins are in Smith's pocket”; 3) Smith “bases his belief in (e) on a count of the coins in Jones's pocket, whom he falsely believes to be the man who will get the job.” (p. 122) His argument in the Brown in Barcelona Case doesn’t even bother to restate the conditions he has stipulated. Right after stipulating that Jones doesn’t own a Ford and that Brown is in Barcelona, he says: “If these two conditions hold, then Smith does not know that (h) [“Either Jones owns a Ford, or Brown is in Barcelona”] is true, even though (i) (h) is true, (ii) Smith does believe that (h) is true, and (iii) Smith is justified in believing that (h) is true.” (p. 123) Well, of course, the two conditions hold, since Gettier has stipulated that they hold, just as he stipulated earlier “that Smith has strong evidence . . . that Jones owns a Ford.” The question in whether Gettier’s conclusion that Smith does not know (h) follows from these stipulated conditions.

Deutsch locates Kripke’s argument for the Gödel Case is his reasoning that in a real-life case like the undeservedly
accredited Peano, a descriptivist theory of reference would entail that if someone who had no association with Peano except the false belief that Peano discovered Peano’s axioms were to say: “Peano discovered Peano’s axioms,” then she would be saying something trivially true rather than false. This is a distinct argument, but it is not a distinct argument for the truth of the intuition elicited by the Gödel Case. That intuition is that the person being referred to by ‘Gödel’ is still Gödel rather than Schmidt. His argument for that intuition is just the end of the case itself: “So, since the man who discovered the incompleteness of arithmetic is in fact Schmidt, we, when talk about ‘Gödel,’ are in fact always referring to Schmidt. But it seems to me that we are not are not. We simply are not.” (p. 84) The argument that Deutsch alludes to is an argument about how we ought to use proper names for purposes of referring rather than an argument about what we actually do.

The premises in Frankfurt’s argument for the truth of the intuition that “since Jones is acting solely from reasons of his own, the fact that Black ensures he cannot act otherwise does not excuse him from moral responsibility” makes explicit what is already implicit in the case itself. Frankfurt has already specified that: “Jones for reasons of his own decides to perform and does perform the very action that Black wants him to perform” and “Black never has to show his hand because Jones for reasons of his own, decides to perform and does perform the very action Black wants him to perform.” His argument then highlights the significance of these givens by pointing out that the fact Jones could not do otherwise “played no role at all in leading to him to act as he did” and that “everything happened just as it would have happened without Black’s presence in the situation and without his readiness to intrude into it.” In subsequent passages he restates these points using still other words. Restatement
of this kind is a legitimate way to clarify and emphasize, but I doubt that it constitutes “evidence for the evidence.”

Second, the arguments Deutsch hails as “evidence for the evidence” are logically incomplete. They are enthymemes. In order to be valid and they require additional premise. In the Gettier cases the missing premise could be a principle of the form: “a justified true belief x is knowledge only if it satisfies condition c.” In the Gödel Case, the premise could be: “Whenever we use a personal name n to refer to someone and that name has been passed down to us through a causal-historical process that originated in the bestowal of that name on an individual person, i, then n refers to i.” In Frankfurt’s case the premise could be: “whenever a person acts solely from reasons of his own, the fact he cannot act otherwise does not excuse him from moral responsibility.”

Of course, such premises have to be argued for, not just stipulated, and attempts to do so have failed to produce unanimity. Epistemologists have quarreled for fifty years over what condition c might be. Philosophers of language differ on when and whether the reference of a personal name depends on the sentence in which that name occurs. Parties to the free will controversy debate the meaning and possibility of acting “solely from reasons of one’s own,” especially if the role of counterfactual intervener is played by natural forces. Whatever the reasons for the absence of such premises, their absence has not detracted from the enduring interest generated by the cases themselves. Indeed, I suspect that these cases continue to be fascinating precisely because they elicit striking intuitions without tying down those intuitions to premises that would have to endure the gauntlet of philosophical scrutiny and criticism.
Third, I take the regress problem seriously. Deutsch, to his credit, does so as well. He says:

[T]he idea is that, ultimately, if any conclusions we reach via argument are to be properly grounded in evidence or justified at all, then there must be some point in the chain of premises, and premises for the premises, and so on, at which we reach propositions that qualify as evidence but are not arrived at via inference from still further premises. This evidential “rock bottom” need not be the premises that a philosopher . . . takes as a starting point in arguing for some conclusion in a paper or book, but the thought is that rock bottom evidence must nevertheless be there, waiting in the wings as it were: otherwise there is no, and cannot be any, inferential evidence for anything. (pp. 124-125)

Despite his acknowledgement that the regress problem needs to be taken seriously, he offers several reasons for thinking that it does not compromise his critique of negative x-phi.

To begin with, he points out that negative x-phi focuses on the use of intuitions as evidence at a stage (e.g. in thought experiments and cases) that is far above rock bottom and repeats his allegation that it misinterprets the evidential role they actually play. He also claims: “the regress problem concerns inferential justification in general. It is not a special problem for philosophy or its methods.” (p. 126) Science, he argues, is no better off than philosophy when it comes to epistemic foundations: “Is there any evidence or argument that would suggest that rock bottom scientific
evidence is, or would be, universally agreed on, or that judgments about such evidence would not vary with truth-irrelevant factors?” (p. 126) He notes, moreover, that if coherentism is true, “[s]ome premises are justified not by inference from further premises but instead by their coherence with other premises.” (p. 127)

I am not persuaded. Deutsch conflates the problem of grounding premises in evidence with the problem of rock bottom foundations and blurs important differences between philosophy and the sciences. The sciences seek to solve problems by finding data that will help to confirm or disconfirm a given hypothesis or decide between alternative hypotheses. They count data as evidentially significant when the data indicate that one hypothesis is better than another (or the null hypothesis) at making predictions. Methodological safeguards such as repeatability of experimental data and the use of control groups diminish, though they do not eliminate, the influence of truth-irrelevant factors. By these means scientists have been able to reach provisional agreement on solutions to many of their problems. Scientists qua scientists do not address the problem of rock bottom foundations: they leave that to philosophers.

Unfortunately, philosophers have not been able to reach agreement—provisional or otherwise—on the solution to the foundations problem, nor, for that matter, on any other central philosophical problem. Nevertheless, they seek to show that some solutions are better others. One of the ways they seek to do this is by appealing to intuitions, and yet they are curiously unsystematic about this practice. They don’t agree on what intuitions are or why they count as evidence. They don’t specify the credentials of intuiters whose intuitions deserved to be counted or tally the frequency of favorable and unfavorable intuitions among
members of that population. Deutsch suggests that this lack of rigor doesn’t matter because intuitions per se are not evidence: they are judgments that need to be argued for. Experimental philosophers, by contrast, take at face value the philosophical practice of treating intuitions as evidence and put that practice to the test by imposing on it the rigor of data-gathering standards in cognitive psychology.

All roads lead back to the same question: Can an intuition “that p” be evidence that p is true? I think the correct answer is: sometimes. Consider the cases discussed above. Each of Gettier’s cases is intended to elicit the intuition that a particular instance of belief, though justified and true, is not knowledge. Deutsch dismisses the relevance of the cross-cultural study “Normativity and Epistemic Intuitions” that Jonathan Weinberg, Shaun Nichols and Stephen Stich published in 2001. This groundbreaking study found that a majority of Western subjects had epistemic intuitions consistent with Gettier’s attempt to refute JTB, while a majority of Asian subjects did not. Weinberg et. al. argue that if their subject groups are representative, “it seems that what counts as knowledge on the banks of the Ganges may does not count as knowledge on the banks of the Mississippi!” (p. xiv) Deutsch counters: “If anything is clear in this area, it is that, if agents in Gettier cases lack knowledge that is a matter that does not depend on which river’s bank one stands upon.” (p. xiv)

Under what conditions would Deutsch be right? He would be right, I think, if knowledge were a natural kind. The truth of the intuition that diamonds are carbon does not vary from culture to culture, even if the frequency of that intuition does. We can be confident of this—until further notice from the community of chemists—because there is virtual unanimity among chemists that carbon is a chemical element (an atom with six protons) that can take a variety
of forms and that diamond crystals are one such form. Philosophers, however, have not been able to agree on a
definition or theory of knowledge, so there is room to
debate whether and in what respects knowledge is a
cultural rather than a natural kind. If knowledge is, at least
in part, a cultural kind, then the frequency of certain
epistemic intuitions in a given culture may be relevant to
what knowledge is.

Can an intuition “that p” be evidence that p is true, where p
is a proposition about how a word is used to refer? Kripke
presents the Gödel Case to illustrate, what he believes to
be, a fact (not a necessary truth) about the use of personal
names by ordinary people. He concedes that one could
commit in the privacy of one’s room to use a personal
name in the descriptivist way but adds that: “In general our
reference depends not just on what we think ourselves, but
on other people in the community, the history of how the
name reached one, and things like that.” (p. 95) Note that
the fact Kripke is trying to establish is akin the facts sought
by lexicographers. Lexicographers gather data on how
competent speakers in a language community use words to
talk about the world with other members of that
community. I doubt that their data-gathering methods
include eliciting intuitions by thought experiments, but they
assume that what a word means is inseparable from the
intuitions of competent speakers about its sense and
reference. We would laugh at a dictionary entry that said
this word means X, though competent speakers don’t use it
that way. Therefore, unlike Deutsch, I see no reason why
x-phi studies “that appeal to the intuitions of competent
speakers about the reference of proper names” (p. 111)
cannot be relevant to an assessment of Kripke’s view.

The intuitions at issue in Frankfurt’s case are, as Frankfurt
acknowledges, moral intuitions. Specifically, they are
judgments of moral responsibility, blameworthiness, and praiseworthiness. Can an intuition “that p” be evidence that p is true, where p is a proposition that a person is morally responsible and blameworthy for an action? If being morally responsible and blameworthy are inseparable from being judged or held responsible and blameworthy, if they are grounded, as Hume might say, in affections of disapprobation within human breasts, then the answer is yes.

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